Again, in *Astr. Nach.*, 3463, he gives notice of two new variables—probably of short period—B.D. + 67°·1124 in Draco, and B.D. + 30°·1329 in Gemini, with respective positions: R.A. 19h. 9m. 54s., Decl. + 67° 2′·4, and R.A. 6h. 37m. 50s., Decl. + 30° 25′.

All the positions are given for the epoch 1855.

Variations in the Spectrum of Nebula in Orion.—Often has severe criticism put scientific facts on a firmer basis, and such might be said to be the case with Dr. Scheiner's doubts concerning Prof. Campbell's observed variations in the spectrum of different regions in the Orion nebula, made in 1893. To test the accuracy of some specific observations, Prof. Campbell has called in the assistance of three of his colleagues (Prof. Schaeberle amongst them), and, with the aid of the 36-inch refractor and an efficient spectroscope, different regions of the nebula have been examined to observe the behaviour of the three principal nebular lines.

The mode of making the observations was to use a coarse micrometer wire, occulting each of the lines in turn, so as to determine the relative brightness of the remaining two, when all three are observable.

The following are the results, which in the main all the observers are agreed upon:—Central part of nebula (Trapezium region): the three nebular lines all conspicuous, the line λ 5007 being the brightest, whilst the lines λ 4959 and λ 4861 are nearly of the same intensity as each other.

In the region surrounding the star Bond, No. 734, the line λ 4861 was the only one visible, the other two lines having disappeared; whilst in the region south-west of the Trapezium all three are visible, but the line λ 4861 is still the brightest.

No doubt photographs will be secured whilst Orion is well situated, and so further establish these observations made visually.

Winnecke's Periodic Comet.—One is reminded on reading the life-work of the late Dr. Winnecke, in the last number of Nature, that had he lived a few weeks longer he would probably have seen another return of the periodic comet which bears his name, for it is due at perihelion on March 20, 1898, but of course will be better situated for observation some time before this. The elements and ephemeris as given by Mr. C. Hillebrand in *Astr. Nach.*, 3447, are as follows:—

Elements.

	26 October. 1897.	15 March, 1898.	
	° ′ ″	° ′ ″	
M =	325 24 26·7	359 3 52·0	
π =	274 14 33·3	274 14 39·0	1900
Ω =	100 53 34·3	100 53 11·5	
i =	16 59 34·4	16 59 33·8	
φ =	45 37 35·6	45 37 14·7	
μ =	608″·3483	608″·5559	

Ephemeris.

1897-98.	R.A. h. m. s.	Decl. ° ′	log *r*.	log Δ.	1 : $r^2\Delta^2$.
Dec. 21	14 38 14	− 1 6·6	0·19028	0·28225	0·113
25	51 6	2 2·3	17921	26885	127
29	15 4 28	2 58·6	16788	25550	142
Jan. 2	18 24	3 55·3	15629	24228	160
6	32 53	4 52·3	14446	22929	179
10	15 47 58	5 49·3	13242	21659	200
14	16 3 40	6 46·1	12016	20432	224
18	20 0	7 42·1	10773	19259	251
22	36 59	8 37·0	09518	18151	280
26	16 54 36	9 30·3	08256	17122	311
30	17 12 49	10 21·3	06993	16184	344
Feb. 3	17 31 41	− 11 9·0	0·05738	0·15349	0·379

The best time for making a search will be early in the morning, shortly before sunrise, from about now till early in February. The comet is never visible to the naked eye, and will at first be faint with the aid of a telescope; its apparent path is in a south-easterly direction through Virgo, Libra, Scorpio, Sagittarius, and Capricorn.

KEKULÉ MEMORIAL LECTURE.

At an extra meeting of the Chemical Society, held on Wednesday, December 15, Prof. F. R. Japp, F.R.S., delivered a memorial lecture in honour of the eminent German chemist, Friedrich August Kekulé, whose death occurred in July 1896. The lecturer said that Kekulé's supreme merit lay in his contributions to theoretical chemistry. His greatest achievements in this department were the doctrine of the linking of atoms in terms of their valency, and, growing out of this, the theory of the structure of organic molecules, both in open-chain and in closed-chain compounds. These were not recondite theories, hidden away in the depths of the science; they were organic chemistry itself, and were learnt by students on their first introduction to the subject. Kekulé acknowledged that his theories were based on Gerhardt's type theory, on Williamson's theory of polyvalent compound radicles and multiple types, and on Odling's theory of mixed types, which was a deduction from Williamson's theory. Less consciously, perhaps, his opinions were influenced by E. Frankland's theory of the valency of elementary atoms, and by Kolbe's speculations on the constitution of organic compounds. Kekulé developed these ideas, which he found scattered throughout the writings of his predecessors, added to them, and welded the whole into the coherent system which formed our present theory of the structure of organic compounds. In Kekulé's model of the carbon atom "the four units of affinity," to quote his own words, "radiate from the sphere representing the atom so that they end in the faces of a tetrahedron." This model was destined to play an important part in the development of theoretical chemistry; it was the foundation of stereochemistry. Kekulé's benzene theory was the crowning achievement, in his hands, of the doctrine of the linking of atoms; it was the most brilliant piece of scientific prediction to be found in the entire range of organic chemistry. What Kekulé wrote in 1865 had since been verified in every essential particular. Not only had the various substitution derivatives been discovered in the number and with the properties required by the theory, but various observations which appeared to contradict this theory had been proved erroneous. Moreover, the theory had shown itself to be capable of boundless development, and there seemed to be no limit to the fruitfulness of Kekulé's conception of closed chains or cycloids. The extensions of the idea, of which extensions Erlenmeyer's naphthalene formula and Dewar's formulæ for pyridine and quinoline were among the earliest instances, had gone on increasing in a rapid geometrical ratio, until, at the present day, the literature dealing with cycloids, although of so recent growth, was more than twice as voluminous as that of the paraffinoids. But even in the undeveloped state of the subject prior to Kekulé's theory, the facts were apparently so intricate and so unconnected that few chemists could claim to have mastered them. The theory appeared; the previously unmarshalled facts fell into their proper places; and not only this, but it was possible to say whether, in any given section of the subject, the facts were complete or only fragmentary. The debt which both chemical science and chemical industry owed to Kekulé's benzene theory was incalculable. As regards the former, three-fourths of modern organic chemistry was, directly or indirectly, the product of this theory; and as to the latter, the industries of the coal-tar colours and the artificial therapeutic agents, in their present form and extension, would be inconceivable without the inspiration and guidance of Kekulé's fertile idea. By the accuracy of his predictions he had done more to inspire a belief in the utility of legitimate hypotheses in chemistry, and had therefore done more for the deductive side of the science than almost any other investigator. His work stood pre-eminent as an example of the power of ideas.

RECENT RESEARCHES ON TERRESTRIAL MAGNETISM.[1]

II.

Up to this point we have regarded the system of magnetic forces in play upon the surface of the earth as constant. I have already hinted that this is not the case, and that the difficulties of our investigation are immensely increased by the fact that all the phenomena with which we deal are in a state of flux. Nothing is fixed from year to year, from day to day, from hour to hour. It is hardly too much to say that at times almost every minute brings with it changes which it is the business of the magnetician to investigate and explain. For the moment, however, I wish to fix attention only upon the secular change to which I have already referred. Not only does the angle which the magnet makes with the geographical meridian vary, but the dip also increases and diminishes in turn.

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[1] The "Rede Lecture" delivered in the Senate House, Cambridge, on June 9, by Prof. A. W. Rücker, F.R.S. (Continued from p. 163.)

An interesting method of depicting these changes has lately been suggested by Dr. Bauer. He imagines the observer to be stationed at the central point of a magnet suspended so that it can turn freely in any direction. To a person thus situated, and capable of continuing his observations through periods far exceeding the range of human life, the north pole of the magnet would appear to describe a curve. It would move sometimes to the right, sometimes to the left, and would simultaneously rise or fall. The facts which have been observed during the last 150 years, and, in some places, for longer periods enable us to draw these curves. At the great majority of the places first investigated the pole of the magnet would always appear to the observer to describe its orbit in the same direction as the hands of a watch, so that on the whole when the needle was to the east of its mean position it would generally be falling, and when to the west rising. Unfortunately, however, this statement is not of universal application, as there are a certain number of stations on the west coast of America where the ordinary movement appears to be reversed. If figures representing a number of these magnetic orbits are arranged, in order from north to south, as in Fig. 3, and if those selected are appropriate to places the longitude of which is not very far distant from that of Greenwich, it is found that in passing from north to south the area enclosed by the figure becomes larger, and that it again diminishes when the equator is passed. In other words, for these stations the rule holds good that the orbital sweep of the needle is greatest near to the equator. There is an another curious peculiarity, namely that for stations of approximately the same latitude in the northern and southern hemispheres, respectively, the curves are larger in the Southern Hemisphere.

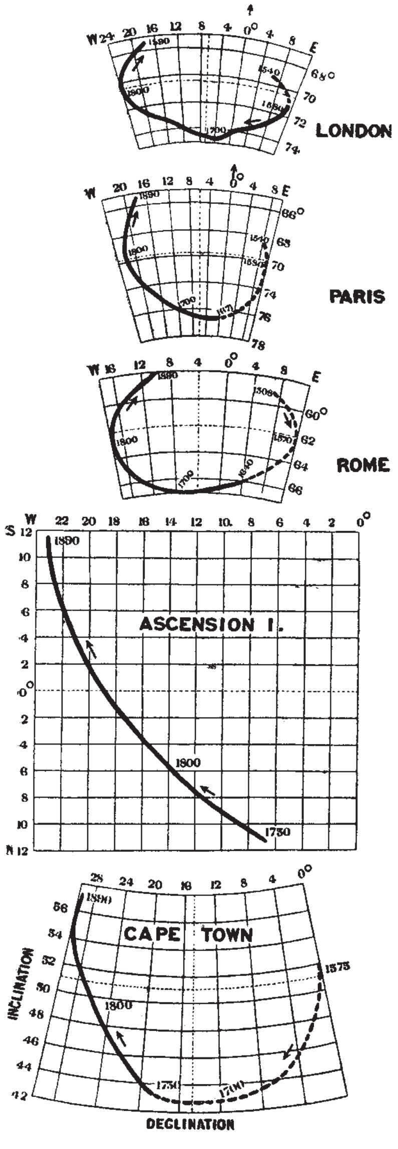


FIG. 3.

If, however, we turn to the American continent, the curves are different, the range of declination is less, and the figures, instead of being of an approximately circular form, become elliptical.

In no case have the phenomena been registered for a sufficiently long time to enable us to complete any one figure. For London we have fairly accurate data for about three centuries, and the earlier observations enable us to extend the curve still further backwards with some probability of accuracy. In the case of our own country we have probably followed the movement of the needle from an extreme easterly position attained in the time of Elizabeth to an extreme westerly position which was reached about 1824, but we have no right to assume that the return journey from west to east will occupy the same time as that which has been watched from east to west.

The first conclusion, then, which I wish to draw from these figures is that they and others like them display what is practically the whole of our knowledge as to the time during which magnetic phenomena go through a complete cycle of change. Calculations have been given by high authorities, which lead to the conclusion that the magnetic pole would perform an orbit round the pole of the globe in about 960 years; but a glance at the curves is sufficient to show how uncertain are the data upon which such estimates are based.

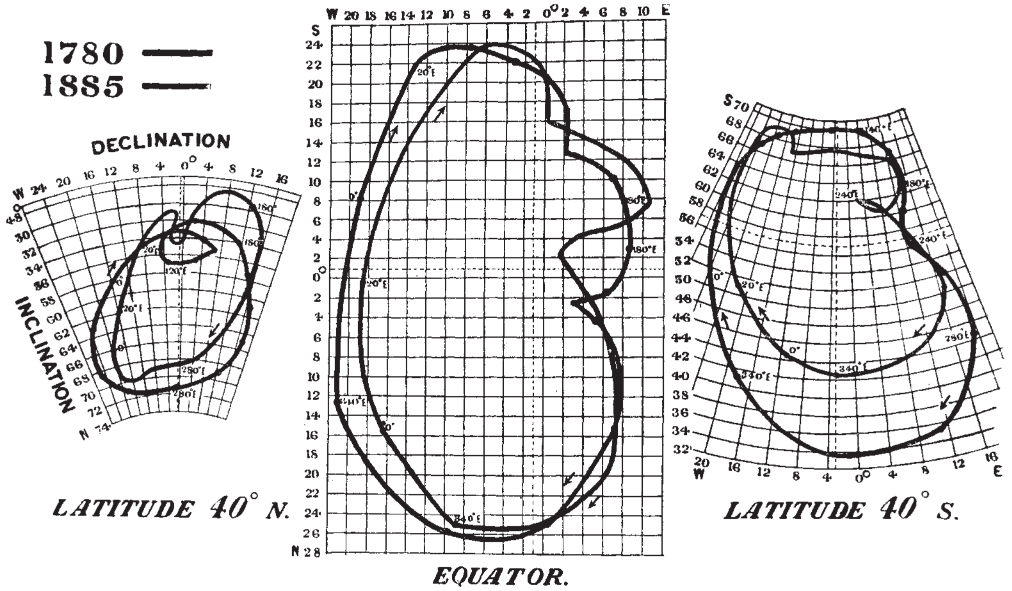
The speed with which the secular orbit is described does not appear to be constant. It may be more or less rapid in the future than in the past. The curves, though so far smooth and continuous, may in the future develop loops or irregularities of various kinds. We may doubt whether all the orbits are described in the same time. It is quite possible that the paths may not return into themselves, or be repeated in successive cycles.

These difficulties may be illustrated by another suggestive device of Dr. Bauer's. Let us suppose for a moment that the principal part of the magnetic system of the earth consists of forces due to currents or magnetic matter which are unchangeable and are arranged symmetrically with respect to the geographical axis. Let there be superimposed upon this another magnetic system describing within the earth and around the geographical axis an orderly orbit completed in an unknown term of years. Let us further suppose that this second system is itself unalterable except in position, so that, as it revolves, the magnitude of the forces is unchanged though the position of the points from which they emanate varies. Under these circumstances we should be able to foretell from the present state of the earth what the future cycle of change would be. If the magnetic needle were placed at some fixed point on the surface of the earth, it might take hundreds of years for the revolving magnetic system to make one complete revolution so as to take up every possible position with regard to the needle. But if the needle were suspended like Mahomet's coffin near to the surface of the earth, and were held there while the globe rotated beneath it, then in a single day the relative positions of the revolving magnetic system and the suspended needle would undergo every possible change, and the pole of the needle would describe in twenty-four hours the path which may in reality be accomplished in a millenium.

Of course we cannot thus suspend a magnet in space, but the same end would be attained by carrying it round the globe along a parallel of latitude, and we have sufficient knowledge of the magnetic conditions of the surface of the earth to determine the curve which the pole of the needle would describe during such a journey.

Three of these curves have been drawn by Dr. Bauer for the equator and for latitudes 40° N. and 40° S., and a glance at the diagram on which they are depicted shows that though they are larger, they bear a general resemblance to the secular curves which portray the movements of the needle at various stations near to the prime meridian. In particular the curious difference in the size of the orbits for equal latitudes north and south of the equator respectively is reproduced. Dr. Bauer has drawn curves for three different dates, of which only those corresponding to two are here reproduced (Fig 4), and of course, if the basis of our argument were unimpeachable, they should be identical. This is not so; there are noticeable, although not very great, differences, but, together with other facts already mentioned, the variations are sufficient to enable us to affirm that the secular change cannot be deduced in this simple fashion from the present magnetic state of the earth

There is one other point with regard to the curves which shows how great must be our caution in arguing from such data. In the lower parts there is a general similarity with the curves deduced from past observations with the magnet at different places near the prime meridian, but in the upper portions, which refer to the future, the curves are more complicated, and they are varied by irregularities and loops of which we have, as yet, had no actual experience. Dr. Bauer has observed most praiseworthy caution in deducing any definite conclusions from these interesting speculations. But even if we refuse to accept his hypothesis that there is a more than accidental connection between the secular curve which the pole of the magnet describes at any one place, and the curve which it would describe if carried round the earth in the latitude of that place, there can be no question that it is possible that irregularities similar to those seen in the one set of curves may in the future occur in the others, and that any deduction which we may make as to the period of the magnetic cycle from the past, may be woefully falsified by future events.



LATITUDE 40° N. EQUATOR. LATITUDE 40° S.

FIG. 4.

It is instructive to compare with this speculation the results obtained by Captain Creak from a comparison of the maps of the magnetic state of the earth drawn by General Sabine and those which Creak himself elaborated with the aid of the observations made by the *Challenger* expedition. The conclusion to which he came was that during the forty years which elapsed between the epochs for which the maps were drawn, the secular change might be best represented by supposing that the magnetic poles were stationary, but that at certain points on the surface of the earth the forces of terrestrial magnetism had been increasing or diminishing. I have before now referred to these interesting observations in public, and I will not dwell on them again, except to remark it is possible that we may have to deal with causes both of the one type and of the other, that within the earth there may be a rotating magnetic system, and that purely local causes may also alter the intensity of the magnetic forces at different places.

Dr. Schuster has recently shown that a motion of the magnetic pole might be produced if the space round the earth were conducting. The conditions under which this would occur cannot be discussed now, but the magnetician has reason to be grateful for any hint as to a possible physical explanation of the secular change.

In addition to the long-protracted changes which have been discussed, the pole of a freely-suspended magnet also describes an orbit from day to day, which is so small, when compared with the secular path, that hitherto I have neglected it. This diurnal change has long been studied. Its magnitude varies from time to time and has been found to be connected with the season of the year and with the sunspot period. On these comparatively well-known facts I will not dwell, but there is one point in connection with the diurnal variation which has recently been attracting attention. For days together the orbits described may be very similar, but this regular motion is frequently disturbed by violent perturbations, and the pole of the magnet moves within a few minutes both to the west and to the east to an extent far exceeding that due to the ordinary diurnal motion, so that the form of the path is widely irregular. Such a phenomenon is called a magnetic storm, and from the fact that such storms occur simultaneously at places which are widely distant it has been thought that it is possible that they are due to some impulse which reaches the earth from the sun. They are also closely connected with displays of the Aurora Borealis. These irregularities make it difficult to determine what is the true normal behaviour of a freely suspended magnet. The most obvious plan for determining the average movement of the needle is to take the mean of its positions at each one of the twenty-four hours for every day in the year. The disturbances just referred to will thus be included in the calculations, but as very great magnetic storms are comparatively rare, the final value will not be very largely affected. This process is very laborious, and exceeds the powers of any except the best equipped observatories. Some time ago, therefore, it was suggested that the calculations should be shortened by the selection of quiet days only, on which it was to be supposed that the behaviour of the needle was normal. Five days in each month were considered sufficient, and by a happy arrangement all the English observatories have agreed that these days shall be chosen by the Astronomer Royal, and that thus the determination of the normal movement of the needle shall be made by all of them from data collected at the same time.

Attention has recently been drawn to the fact that, whatever the convenience of this five-day method may be, it leads to the conclusion that at the end of a quiet day, the needle does not return to the position which it occupied at the beginning. This point has been carefully investigated by Dr. Chree, and may be illustrated by means of the curve which I have drawn from the Kew Observatory Report for the summer of 1895, the scale of which is many times larger than that of the secular orbits already depicted (Fig. 5). The upward and downward movement of the needle is much smaller than the displacement east and west, so that an elongated figure is produced, but the interesting point is that it is not a closed curve; the two ends do not meet, but are separated by a very appreciable interval. It would, of course, be an easy explanation of this fact, if we could attribute it to the secular change. Just as the moon, though at the end of a month in the same position with respect to the earth as it was at the beginning, is much further advanced in the earth's orbit, so the diurnal magnetic variation must accommodate itself to the larger secular movement of which it is a subordinate part. But this explanation alone will not suffice. It is true that during the quiet days the movement of the needle in its secular path continues, but there is a good deal of evidence to show that it is of more than average speed. This is especially true of the horizontal force, which is gradually increasing and increases with remarkable rapidity on quiet days. Hence the secular movement appears to be checked by the storms. The comparatively rapid progress which has been made in quieter times, being retarded and even reversed during the periods of irregular motion which I have described.

It is true that General Sabine many years ago showed that magnetic storms do not act equally in both directions upon the needle, and that thus the phenomena which I am now describing can hardly be said to be recently discovered; but the method of presenting it which has been adopted by Dr. Chree, and which I have slightly modified by including in the diagram the variations both of declination and of dip, certainly places the facts before us in a novel and a striking light. Of what the cause of the sudden check which the needle receives during the magnetic storms may be, we can as yet say nothing. It is one of the puzzles which has yet to be unravelled.

The last point to which I will refer is one upon which more definite results have been obtained. Terrestrial magnetism is connected with phenomena which occur in the sun, with the Aurora Borealis in the upper atmosphere, and with the earth currents which traverse the soil. I have now to draw your attention to its relation to geology.

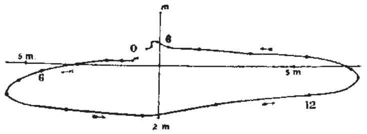


FIG. 5.

It has long been known that just as the great secular variation of the magnet is accompanied by minor diurnal changes, so the large alterations in the direction of the compass and dipping needle, which are observed when we move from place to place on the surface of the earth, are affected by irregularities which are apparently due to purely local causes. Thus the declination is greater in Ireland than in England; but the increase is not uniform as we pass from one country to the other. In fact in some districts an abnormally large increase is followed by a decrease.

These curious inequalities must be due to local disturbing forces, and the large number of observations which have been made in this country have enabled us to determine with more than usual accuracy the magnitude and direction which the magnetic forces would assume if they were undisturbed by any local cause, and from the difference between things as they then would be and things as they actually are, we can calculate the magnitude and direction of the disturbing forces themselves. When these are represented on a map, it is found that there are large districts of the country in which the disturbing horizontal forces act in the same direction; in one region the north pole of the needle will be deflected to the east, in another to the west, and, as we pass from one of these districts to the other, we always find that at the boundary the downward vertical force on the north pole of the needle reaches a maximum value. We are thus able to draw upon the map lines towards which the north pole of the needle is attracted. It is found that the exact position of these can be determined with considerable accuracy, and that the lines can be traced without any possible doubt through distances amounting, in some instances, to a couple of hundred miles. The key to this curious fact is probably furnished by observations in the neighbourhood of great masses of basalt or other magnetic rocks. If these were magnetised by the induction of the earth's magnetic field, the upper portions of them would in this hemisphere attract the north pole of the needle; and it is found that where large masses of basalt exist, as in Antrim, in the Scotch coal-fields, in North Wales, and elsewhere, the north pole of the needle is, as a matter of fact, attracted towards them from distances which may amount to fifty miles. The thickness of the sheets of basalt is in most cases too small to furnish a complete explanation of the observed facts, but it is quite possible that these surface layers of magnetic matter are merely indications of underground protuberances of similar rocks from which the surface sheets have been extruded. At all events, there is no possible doubt of the fact that where large masses of basalt occur, the north pole of the needle tends to move towards them.

There are other regions where the attractions are manifest, but where, nevertheless, no magnetic rocks occur upon the surface; but it is most probable that the cause is the same, and that it is due to the mere accident of denudation that in one case we can, and in the other we cannot, point to the magnetic rocks to which the anomalous behaviour of the compass is due. If this be so, it is certainly interesting that magnetic observations should enable us to penetrate to depths which the geologist cannot otherwise reach, and that the lines which we draw upon the surface of the map, as those to which the north pole is attracted, may, in fact, roughly represent the ridge-lines of concealed masses of magnetic rocks, which are the foundations upon which the deposits studied by the geologist have been laid.

There is some ground for thinking that if these great underground wrinkles exist, they have affected the rocks which are superposed upon them, especially those which are of a comparatively early date. As a general rule, if older rocks appear in the midst of newer ones, the pole of the magnet will be attracted towards the protruding mass; but this rule holds good only of the rocks of carboniferous or pre-carboniferous age, and does not apply to later deposits. As a striking example, I may remind you that the Pennine Range—which is sometimes called the "Backbone of England"—is a mass of millstone grit rising amid younger rocks. Down this a well-marked magnetic ridge-line runs. Similarly, in the neighbourhood of Birmingham, the Dudley and Nuneaton coal-fields are surrounded by more modern deposits. A curious horse-shoe shaped ridge line connects these two, and then runs south to Reading, which is, magnetically speaking, one of the most important towns in the kingdom. East and west from Dover to Milford Haven, and then across the Irish Channel to Wexford, runs a ridge of the older rocks called by geologists the Palæozoic Ridge, concealed in many places by newer deposits. Hollowed out in this are the South Wales and Forest of Dean coal-fields, and in another hollow within it lies the coal which has recently been discovered at Dover. Closely following this protruding mass of the older rocks is a magnetic ridge-line which passes through Reading, and we thus have a magnetic connection between the anticlinals of Warwickshire and the Palæozoic Ridge. From the neighbourhood of Reading also another magnetic ridge-line runs southwards, entering the channel near Chichester. M. Moureaux, who with most untiring energy has for many years been investigating single-handed the magnetic constitution of France, has discovered the continuation of this line on the French coast near Dieppe, and has traced it through the north of France to some fifty miles south of Paris. The energy which is now being displayed by magnetic surveyors in many countries will, no doubt before long, prove that the network of these magnetic ridge-lines is universal; and the relations between them and the geological conformation of the countries in which they lie will be so studied that our inductions will be based upon an adequate knowledge of facts.

This, at all events, we may hope, that amid the flux and change of magnetic forces with which we have so largely been occupied, we may have found in these ridge-lines physical features of the country as permanent as the hills themselves.

And now that I draw near to the end of my lecture, I cannot but feel that it has to a certain extent been wanting in organic unity. It is sometimes possible to unfold the story of a scientific advance in strictly dramatic form. The question posed, the oracle consulted, and the answer given, might well form the titles of the three acts in the modern miracle-play of scientific discovery. But the drama has its conventions, and even those authors who boast of their realism, too often falsify by overemphasising the points on which they chiefly dwell. If we would know life we must learn it not from the stage-box or dress circle, but by mingling with mankind. If we would know what scientific work is, we must not be content with listening only to skilfully-told tales of scientific triumphs, but must penetrate into the observatory or laboratory, where the fear of failure, and the uncertainty of long watching and waiting, are at least as frequent visitors as the assured forecast of success.

To-day, therefore, I have dealt with problems which are still surrounded by doubt and difficulty, with questions which can only be answered by the combined work of many men, it may be of many generations. It is true that on some of these matters we are gradually acquiring definite knowledge. That earth-air currents, if they exist at all, are very minute in north-western Europe; that the diurnal variation on quiet days is not necessarily to be regarded as normal; that local magnetic disturbances are due to forces so wide in their range that it is worth while to study them; these are all facts about which we were in doubt a few years ago, and on which we are in doubt no longer. But greater questions which lie behind these are still unanswered, and if I have ventured to deal with difficulties as well as with certainties, it has been because I have wished to give you a correct idea of current scientific thought on the subject of terrestrial magnetism.

MR. MERRIFIELD'S EXPERIMENTS ON THE RELATION OF TEMPERATURE TO VARIATION.

FOR the last ten or eleven years Mr. Frederic Merrifield, of Brighton, has been conducting a most elaborate and extensive series of experiments in the rearing of lepidopterous insects under various conditions of temperature. The results obtained by him are of high interest, both in themselves and also in reference to similar experiments independently undertaken by Dorfmeister, Weismann, Standfuss and others, some of which have already been noticed in the pages of NATURE. It is proposed to give here a short general account of the chief of Mr. Merrifield's experiments, with figures of some of the main results obtained. For full details the reader is referred to the original accounts which have appeared from time to time in the *Transactions* of the Entomological Society of London.

Experiments in 1887.—The first experiments undertaken had the object of supplying data for an inquiry by Mr. Francis Galton on the subject of heredity. It was anticipated by Mr. Galton that "the experiments would elicit incidentally many interesting results, some perhaps quite disconnected with the objects immediately in view." This anticipation was fully borne out by the facts.

The first species taken for experiment were two geometer moths, *Selenia bilunaria*, Esp. (*illunaria*, Hb.), and *S. tetralunaria*, Hufn. (*illustraria*, Hb.), both normally double-brooded in this country. Larvæ of both species were reared from eggs laid by females of the spring emergence; some of the moths resulting from these were selected for pairing according to size, three classes being formed, of maximum, medium and minimum expanse of wing, and the rearing of fresh generations was continued. Some of both species were fed up in the open air; these showed nothing remarkable. Others (of *S. bilunaria*) were kept during all their stages at a temperature of about 80° F., which had the effect of considerably accelerating their development. Five generations of *S. bilunaria* (counting the moths of the spring emergence as the first generation) were thus produced in the course of the year. These bred moths were all of the summer or *juliaria* form, and the females were always larger than the males, which is in accordance with the rule for the natural summer brood. The pairs selected for maximum and minimum expanse of wing produced no fertile eggs after the third generation. The fourth generation consisted entirely of the offspring of one of the medium-sized pairs of the third, and from these a selection was again made as before. The resulting moths of the fifth generation emerged in December and January, showing signs of deterioration. Only one of this brood laid fertile eggs, and these failed to hatch. The average size of the moths increased continuously up to the fourth, but diminished in the fifth generation.

Experiments in 1888.—The summer of 1888 was cold and wet, and the moths of both the selected species reared from larvæ kept in the open air showed signs of degeneration. That this was not due to the domestication of their progenitors appeared from the fact that a wild strain of *S. tetralunaria* behaved in the same manner.

It was observed in the case of *S. bilunaria* that the specimens produced from larvæ and pupæ that had been kept at about 80° F. showed a warmer colouring and fewer spots than those reared throughout their stages at ordinary temperatures. The same was found to be true in a still more marked degree of another species of geometer moth, *Ennomos autumnaria*, Wernb., and also, though to a less extent, of *S. tetralunaria*.

Experiments on the pupæ of *S. tetralunaria* led to interesting results. It was found that moths derived from pupæ of the summer brood, first retarded in development by freezing, though ultimately forced for a few days, tended, especially in the females, to assume the appearance of the comparatively dark spring emergence. Some larvæ of the same summer brood and their resulting pupæ were forced from the beginning, with the result that the same batch split into two divisions, the first of these feeding up rapidly and emerging, during the same season, with the summer colouring; the second taking much longer, and attempting to lie over for the winter as pupæ. A continuance of the forcing process brought out some of the latter in November and December, with the summer colouring. Others were exposed out of doors from November 7 to January 1, when they were brought into the house and again forced. The moths from this latter group came out in January and February, and were all of the spring colouring. Inasmuch as the continuously forced batch gave the summer form as late as December, it would appear that retardation alone is not in all cases sufficient to determine the assumption of the spring coloration. On the other hand, some pupæ of *S. tetralunaria* reared by Mr. Jenner, which were similarly trying to lie over for the winter, produced under forcing a series of intermediate forms becoming on the whole more and more like the spring type with the length of time that elapsed before their emergence. Here there was no exposure to winter cold; but only retardation from constitutional causes.

Mr. Merrifield remarks that the case of *S. tetralunaria* shows that the alternate succession of the two forms is not a necessary accompaniment of seasonal dimorphism. The same appears from Weismann's earlier experiments, in which, under appropriate conditions of temperature, the summer form *Vanessa prorsa*, L., was found to give rise in the next generation to *V. prorsa* instead of to *V. levana*, L. (the spring form).

The different reaction of members of the same brood of *S. tetralunaria* to the same conditions of temperature is interesting as an example of what may perhaps be called "physiological dimorphism"—a principle which there is reason to think is widely prevalent in nature, and which probably favours the survival of those species that exhibit it.

Experiments in 1889.—These had two main objects, the first being to determine the amount of exposure to cold that could be borne in the different stages, the second to ascertain more definitely the effect upon the perfect insect of temperature conditions applied during the immature periods.

Under the first head it was found that the eggs of both species of *Selenia* were injured by protracted icing; a temperature of 80° to 90° F., on the other hand, did no harm and quickened their development. Some eggs of *S. tetralunaria* gave another good example of physiological dimorphism. Thirty eggs were iced for seventeen days. On their removal from the ice-box, two hatched at once; none of the rest yielded larvæ until from eleven to thirteen days afterwards, when nineteen of them also hatched.

Icing the larvæ of *S. tetralunaria* was found to be rapidly fatal, and cooling injurious. The older larvæ stood cold better than the young ones. Larvæ of *S. bilunaria*, *S. tetralunaria*, *E. autumnaria* and *E. alniaria*, L., all endured a continuous temperature of 80° F. or a little more without apparent injury; but one of 90° to 100° F. was very detrimental.

Further experiments showed that no harm resulted from icing for moderate periods the pupating larvæ and pupæ of *S. tetralunaria* and the pupæ of *E. alniaria*, nor from cooling to about 47° F. the pupating larvæ or pupæ of *E. autumnaria*.

Under the second head some interesting conclusions were arrived at with *E. autumnaria*, *E. alniaria* and *S. tetralunaria*. Eggs from a single pair of the first-named species were divided into batches, and larvæ and pupæ of each batch were brought up under carefully regulated conditions of temperature. The